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ON THE PRINCIPLE OF CONVERGENCE IN  
ETHNOLOGY<sup>1</sup>

BY ROBERT H. LOWIE

GRAEBNER'S POSITION

IN a recent work on the methods of ethnology,<sup>2</sup> Dr. Graebner once more expounds the theoretical position familiar to readers of his former writings.<sup>3</sup> The central problem of ethnology is for him the determination of cultural connections. Resemblances in culture must be primarily accounted for by historical connection, — in the first place, because the existence of such connection stands unchallenged for a large part of the phenomena; secondly, because there are no objective criteria of independent development. Lack of historical relationship cannot be established by the most intense feeling that such a relationship is improbable, for this feeling is of a purely subjective character. Neither can the absence of proof for historical connection be interpreted as a stringent demonstration that an historical relationship does not exist. It is indeed conceivable, that, after determining all cultural relationships, we may still be confronted with independent partial similarities; but obviously this conclusion would result, not from the application of definite criteria of independent evolution, but solely from the non-applicability of the criteria of cultural connection. "So bleibt denn als erstes und Grundproblem der Ethnologie wie der ganzen Kulturgeschichte die Herausarbeitung der Kulturbeziehungen."

What, then, are the criteria of cultural connection? Two such are recognized by Graebner, — the criterion of form, that is, of the coincidence of characteristics not necessarily resulting from the nature of the objects compared: and the criterion of quantitative coincidence. In innumerable cases the form-criterion is self-sufficient. Nevertheless, Graebner notes instances of its misapplication, through fanciful affiliation of heterogeneous forms. Here, it seems, the quantitative criterion should have been used; that is to say, as it is one of the cardinal doctrines of Graebner's philosophy of ethnology that the diffusion of isolated cultural elements — even of myths — is impossible (*kulturgeschichtliches Nonsense*), the doubtful parallelism of two forms can be immediately established if they are recognized as elements of the same or related cultural complexes. So far as continuous areas are concerned, these criteria have not been challenged: they are gen-

<sup>1</sup> Presented at the annual meeting of the American Folk-Lore Society in Washington, Dec. 28, 1911.

<sup>2</sup> *Methode der Ethnologie* (Carl Winter's Universitätsbuchhandlung, Heidelberg, 1911).

<sup>3</sup> More particularly, "Die melanésische Bogenkultur und ihre Verwandten," *Anthropos*, iv (1909), pp. 726–780, 998–1032.

erally employed in establishing linguistic relationship, and have proved valid in the study of European culture. Graebner sees no reason for limiting the criteria to continuous areas: he does not hesitate, for example, to use them as proofs for a far-reaching connection between Old-World and New-World culture. The only objection advanced against such applications of the criteria has been the improbability, under primitive conditions, of diffusion over the tremendous distances dealt with. On the one hand, this argument is refuted by the migrations of the Malayo-Polynesians and the occurrence of Asiatic tales in South America. But, in addition, the contrary argument may be strengthened by two auxiliary principles. The supposed lack of continuity between two areas may prove deceptive. There may be found cultural features bridging the geographical gap between the areas compared (continuity-criterion); and there may be such a diffusion of cultural elements, that geographical proximity varies directly with the degree of cultural relationship (criterion of form-variation), — a result manifestly not to be expected on the theory of independent evolution of parallel forms.<sup>1</sup>

The foregoing account already describes by implication Graebner's position on the subject of convergent evolution. From his point of view, it matters little whether similarities are believed to result from a psychology common to mankind or from the convergence of originally distinct phenomena. In either case, there is an assumption of independent development; and as positive criteria of independent development are, according to Graebner, non-existent, both theories are on a methodologically inferior plane as compared with the doctrine of historical connection. In particular, Graebner criticises Ehrenreich's definition of "convergent evolution" as the result of similar environment, similar psychology, and similar cultural conditions. Similarities in natural conditions, he contends, have been considerably overestimated. The psychology of different branches of mankind shows as much differentiation as their physical traits. As a matter of fact, the psychological unity of mankind, which is invoked to explain cultural resemblances, has really been inferred only from the observed resemblances. If peoples of distinct geographical areas reveal far-reaching psychical resemblances, the question arises whether these are not ultimately due to genetic relationship or cultural contact. So far as the similarity of cultural conditions is concerned, Graebner insists that, if independent development be assumed, similarity of cultural conditions could result solely from the natural environment, and that similarity of cultural conditions would presuppose a high degree of psychical resemblance. Against Ehrenreich's statement, that in spite of various parallels with Old-World culture, the culture of America bears

<sup>1</sup> Graebner, *l. c.*, pp. 94-125.

a distinctively American stamp, Graebner declares that it is not clear how heterogeneous cultural conditions could lead to parallels, which, according to Ehrenreich, must be due to a *similar* cultural environment. An *a fortiori* argument is used to clinch the discussion. European civilization has developed a remarkable similarity of cultural *milieu*. Nevertheless the number of well-authenticated instances of independent parallel development is exceedingly small. In the majority of instances we find merely combinations of thoughts and motives already extant in the culture common to authors, inventors, or thinkers. But even the residual cases lose their force as to convergent development among primitive races: for, on the one hand, these modern instances rest on a peculiarity of modern culture, — the conscious striving for progressive development; on the other, the same thought may indeed be *conceived* twice, but the literature of science indicates that the same thought does not necessarily become socially and culturally significant in more than one case. If a cultural similarity resting on close genetic relationship has produced so small a number of independent parallels of social significance, it may reasonably be doubted whether the relative psychological unity of mankind, and the resemblance of natural conditions, could produce such absolute identity of culture as to result not merely in the conception, but in the social acceptance and further development, of the same thoughts.

Two questions confront the reader in connection with the views presented above. In the first place, does Dr. Graebner correctly define the logical standing of the antagonistic theories of independent development and genetic or cultural relationship? Secondly, does Dr. Graebner grasp the essentials of the doctrine of convergence as it has been employed in ethnological practice? The following pages will be devoted to an examination of these questions.

#### LOGICAL STANDING OF THE RIVAL THEORIES

The supposed methodological superiority of the theory of contact and relationship rests, as indicated above, on the assumption that it is distinguished by positive, objective criteria, while the rival theory lacks such criteria.<sup>1</sup> Indeed, the argument that independently evolved cultural similarities could be detected only by the non-applicability of Graebner's criteria (p. 107) involves the strongest conviction that criteria of independent development not only have not been found, but that it is impossible to discover them.

In the first place, the objectivity of Graebner's criteria is in large measure illusory. He himself points out that the form-criterion is liable to fanciful subjective interpretations (p. 118). In all doubtful

<sup>1</sup> This point of view also appears in Graebner's brief reply to a critique by Haberlandt, *Petermanns Mitteilungen*, 1911, pp. 228-230.

cases, however, he counsels testing by the second, unconditionally objective (*unbedingt objektiven*) criterion of quantity. It may at once be admitted that this criterion does provide a quantitative measure for the degree of relationship between two cultural complexes. This relationship, however, cannot be established except by demonstrating the relationship of corresponding elements in the two complexes. Each equation can be made only by the application of the form-criterion. In each particular comparison there will thus admittedly be a subjective factor, hence it is quite illogical to argue that a summation of parallels will eliminate the subjective element. Apart from this, what we know of the psychology of investigation does not justify us in the belief that a student who discovers intensive morphological resemblances — though other investigators fail to note them — would ever feel the necessity of resorting to a test by another criterion; and, if he did, he doubtless would have little difficulty in propping up his fanciful parallel by others not less whimsical. Indeed, the quantitative test leads to curious results in Graebner's own case. Against Haberlandt, — who reproaches him with classifying together such diverse objects as the "male" and the "female" spear-thrower, nay, even the Maori sling-stick, — Graebner urges that, if a complex has once been established on the basis of well-defined elements, even a morphologically indeterminate element, such as the spear-thrower, must be regarded as part of the complex, provided its distribution coincide with that of the other elements.<sup>1</sup> This is undoubtedly a vicious principle. From the identity of even an indefinitely large number of corresponding elements in two series it does not follow that certain other associated elements are genuine parallels and must be brought into a genetic relationship. The "male" and the "female" spear-thrower might reasonably be grouped together as conceivable differentiations from a common prototype; but to argue that so heterogeneous an object as the sling-stick is related to them if it occurs in a similar combination of elements, is not testing the criterion of form, but sacrificing it.

While Graebner's criteria of genetic relationship are thus found to lack the strictly objective character claimed for them, independent development need not be defended on purely subjective grounds, even where a stringent demonstration is impossible. Graebner criticises Ehrenreich for holding that the same mythological ideas may develop independently a great number of times from universally observable natural phenomena.<sup>2</sup> This, he contends, is an *a priori* position lacking in sanity, because from the ready conceivability of independent development we cannot infer the *fact* of independent development (p. 97);

<sup>1</sup> *Petermanns Mitteilungen*, 1911, p. 229. Graebner, of course, does not neglect the differences in spear-thrower types except in his theoretical speculations (see *Anthropos*, iv, p. 736).

<sup>2</sup> *Allgemeine Mythologie*, p. 266.

that is to say, Graebner considers the theory of independent development inferior, because it leaves the door open to the arbitrary individual judgment of psychological probability. Now, it may at once be admitted that no amount of psychological investigation can actually demonstrate that two given cultural phenomena, possessing as they do the unique character distinctive of historical happenings, originated independently. A demonstration could be given only if we knew the actual history, which we generally do not. As a matter of fact, however, the theory of independent development is not one whit worse off in this respect than its rival theory; for it is an utterly mistaken notion that the psychological factor is excluded by the assumption of cultural relations. The comparison of form can never do more than establish the identity of forms; that such identity is to be explained by a genetic relationship is an hypothesis of varying degrees of probability. That the details of the crutch-shaped Melanesian paddle should occur in South America is to Dr. Graebner a sufficient proof of common origin (p. 145). Why? Because he cannot conceive how such similarity could result independently. But what is inconceivable for him is perfectly conceivable for Ehrenreich and others. From the inconceivability of independent development by a single student we certainly cannot infer the fact of a common origin. We are dealing with probabilities, not with certainties in either case; the only point is to increase the probability of either theory, and here I cannot find that the doctrine of independent development is in a less favorable position. It seems to me, on the contrary, that a number of observations in individual psychology, as well as a number of social facts, well-nigh establish the independent development of certain simple cultural traits; and that in other cases the probability of such development, while not as yet determined, can be readily investigated at the present time.

As an example of the former kind I should regard certain observations on the re-actions of children in the dark. If the widespread fear of the dark which enters into primitive belief were exclusively the result of tradition, it might be reasonably argued that it had developed from the same source of origin. This theory, however, becomes improbable as soon as we find that the distinctive feeling of uncanniness appears in equal force where all traditional beliefs tending to foster dread of the dark have been rigorously excluded from the child's curriculum.<sup>1</sup> An element not altogether negligible in primitive belief is thus shown to be an element of our psycho-physical constitution. The psychology of dreams furnishes additional material bearing on the question. If certain physiological conditions, say retinal irritations, are regularly correlated with certain dream images which coincide

<sup>1</sup> Mach, *Die Analyse der Empfindungen*, 1906, p. 62. These observations are confirmed by Dr. Petrunkevitch in an oral communication to the present writer.

with widespread mythological conceptions, then such conditions must be considered as constituting a *vera causa* for the explanation of the mythological ideas. Thus, the widespread conception of a grotesquely distorted countenance may be plausibly traced to Wundt's "*Fratzenträume*." Of course, we do not know, and never shall be able to know with certainty, that these dreams formed the foundation of the corresponding beliefs. But to disregard them entirely, to deny that they affect the merits of the case, would be to indulge in that form of sterile hypercriticism with which Graebner not infrequently reproaches his own opponents. In other directions, systematic observations could at least be planned and instituted. For example, psychological child-study might establish the fact that children of different countries re-act in an essentially similar way on the every-day phenomena observable in the heavens. With the same reservations as before, due to the unique character of historical happenings, we should then be justified in attaching a high degree of probability to Ehrenreich's conjecture as to the independent origin of simple nature myths. In other fields, the study of individual psychology from this point of view might present greater practical difficulties: it might, for example, prove impossible to disentangle the influence of traditional art-forms in an inquiry into the development of drawing and design. On the other hand, the inquiry into types of association, such as Galton was the first to conduct on a large scale, seems full of promise, especially so far as color and number symbolism are concerned. The contention that an apparently very odd association common to two distinct regions must have travelled from one to the other, must immediately lose its force if we find the same association arising with a certain frequency among ourselves. The objection might indeed be raised, that, in order to become a cultural phenomenon, the individual association would have to be socialized; this would, however, apply in equal measure on the supposition of borrowing.

So far, then, as the objectivity of the criteria is concerned, the inferiority of the theory of independent development stands unproved. In determining genetic relationship on the ground of formal resemblance, the influence of the personal equation is unavoidable; on the other hand, the arbitrariness of speculations on independent development can be limited by the results of scientific (as opposed to popular) psychology.

If there is any difference in the value of the two theories, it must rest on the alleged absence of historical proofs for independent development, in the face of the universally admitted existence of such proofs for historical connection. It remains to be shown that this allegation is erroneous, that there exist unexceptionable instances of convergent evolution. For this purpose it is necessary to examine somewhat more closely the concept of convergence.

## DEFINITION OF "CONVERGENCE"

The fundamental error in Graebner's critique of convergent evolution lies in the fact that it entirely ignores the group of phenomena to which the principle criticised has been most successfully applied. Taking into account only Ehrenreich's *definitions* of "convergence," and disregarding completely Ehrenreich's further remarks on the subject, Graebner is led to reject the theory because, for the explanation of identities, it seems to involve the assumption of a mystic psychological unity (p. 145).

To be sure, it must be admitted that, if we found *exact* parallels of very complicated phenomena, their occurrence in two areas, no matter how widely separated, could not reasonably be explained by convergence. Let us assume for a moment that we found on the northwest coast of America a social system duplicating such Australian elements as four-class exogamy, belief in lineal descent from the totem, elaborate rites for the multiplication of totems, and the like. If this were the fact, an explanation by the psychic unity of mankind would be lamentably deficient, as may readily be shown by examination of a concrete case. Ehrenreich writes, "Wo gleiche Geistesanlage sich vereint mit Gleichheit der Wirtschaftsform und der gesellschaftlichen Stufe, wird die Cultur im Allgemeinen überall einen gleichen Charakter, einen gleichen Typus tragen, und wir dürfen uns nicht wundern, wenn solche gleiche Typen auch in Einzelheiten grosse Übereinstimmung zeigen und Convergenzen hervorbringen."<sup>1</sup> Let us test the explanatory value of the principle, as thus defined, by a single example. Ehrenreich finds a surprising resemblance between the Dukduk masks of New Britain and the Fish-Dance masks of the Karaya, as well as between the correlated usages. Granting the resemblance, nay, even the exact identity, of the features in question, what meaning can we associate with the statement that the parallel is due to psychic resemblance linked with like economic and sociological conditions? The identity to be explained is not found except among the two above-mentioned representatives of two distinct racial types. What are the psychic traits and cultural conditions common to these two tribes, which are *not shared by those of their geographical neighbors and racial congeners lacking the cultural homologies under discussion?* The principle of continuity is in fact not less essential to a sane theory of independent development than to a sane theory of transmission. There is at least no logical difficulty in assuming that certain laws of evolution are immanent in human society, and must lead *everywhere* to the same results. But to say that psychic affinity and cultural similarity have

<sup>1</sup> "Zur Frage der Beurtheilung und Bewerthung ethnographischer Analogien," *Correspondenz-Blatt der deutschen Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, 1903, pp. 176-180.

produced in two or in a few instances the same result, is logically admissible only if it be shown at the same time for what specific reasons the same result is not noticeable in all other cases, even where psychic affinity is re-enforced by racial relationship, and cultural affinity by geographical and historical contact. So far, then, as Graebner's attack is directed against Ehrenreich's explanation of supposed identities, it is entirely justified: such an explanation is indeed nothing but a mystification. Granted the existence of identities, they are inexplicable.

But the entire aspect of the question changes if we do not interpret the given parallels as identical or homologous, but merely as analogous. In the brief but profound paper quoted above, Ehrenreich has treated this problem with the greatest possible clearness. Over and above what he regards as genuine convergences, he distinguishes "false analogies," due to the inadequacy of our knowledge, to the premature classification of diverse traits under the same concept, labelled with the same catch-word. It is merely necessary to conceive all parallels of any degree of complexity as "false analogies," — to explain them as Ehrenreich himself explains, in exemplary manner, the various forms of totemism, of the belief in metempsychosis, of the swastika and eye-ornament, — and the mystical element in the theory of convergence disappears. The observation of similarities, especially in the absence of obvious paths of diffusion, then leads directly to the query whether the similarities are not purely classificatory, and hence, from the standpoint of genetic relationship, illusory.

In a review of Graebner's recent book,<sup>1</sup> which has been published since the writing of the preceding paragraphs, Professor Boas says, "Nobody claims that convergence means an absolute identity of phenomena derived from heterogeneous sources; but we think we have ample proof to show that the most diverse ethnic phenomena, when subject to similar psychical conditions, or when referring to similar activities, will give similar results (not equal results), which we group naturally under the same category when viewed, not from an historical standpoint, but from that of psychology, technology, or other similar standpoints. The problem of convergence lies in the correct interpretation of the significance of ethnic phenomena that are apparently identical, but in many respects distinct; and also in the tendency of distinct phenomena to become psychologically similar, due to the shifting of some of their concomitant elements — as when the reason for a taboo shifts from the ground of religious avoidance to that of mere custom" (*l. c.*, p. 807). As is shown by a preceding quotation from Ehrenreich, Professor Boas goes too far in his initial statement, for Ehrenreich's conception of genuine convergence does practically involve a belief in an absolute identity derived from heterogeneous

<sup>1</sup> *Science*, 1911, pp. 804-810.

sources; but his utterance indicates that in America, at all events, convergence has been treated in a manner which entirely escapes Graebner's attention.

It is now necessary to discuss convergence as resulting from modes of classification, to show what form of classification gives rise to the appearance of identical results from diverse sources, and to illustrate the point by a number of special instances.

#### PREMATURE CLASSIFICATION

Premature classification appears in ethnological literature in two principal forms: the ethnologist may either infer from the undoubted identity of certain elements in two different complexes that the complexes themselves are identical; or he may fancy identity of elements or complexes where none exists. The first type of premature classification has wrought considerable mischief in the consideration of ceremonial complexes, such as the Midewiwin and the Sun Dance. The psychology of this fallacy is not unlike that of illusions. A complex such as the Midewiwin is described for some particular tribe; and some conspicuous feature, say, the shooting-ritual, acquires a symbolic function; so that whenever this feature appears in another tribe, it is at once supposed to indicate the presence of the residual elements of the complex first described. This would indeed be a justifiable inference, if a complex invariably represented a quasi-organic unit; but this is precisely what is not ordinarily the case. For example, Dr. Radin has recently shown<sup>1</sup> that the Midewiwin of the Winnebago and that of the Central Algonkin are not identical, because in each there has been a secondary association between the common elements and a preponderant group of specific elements, which in large measure can be shown to result from the specific character of Central Algonkin and Winnebago culture respectively. I have suggested elsewhere<sup>2</sup> that what Dr. Radin has successfully demonstrated for the Midewiwin applies in like measure to the Sun Dance of the Plains tribes. We cannot reduce to a common prototype the various forms in which the ceremonies grouped under this catch-word appear. All we can do is to ascertain the relatively few common elements which have acquired the symbolic function mentioned, and to investigate their varying combinations in different cases.

It is clear that the form of erroneous classification treated above, however large it may loom in ethnological discussion, has nothing to do with convergent evolution; for in the cases mentioned the genetic

<sup>1</sup> "The Ritual and Significance of the Winnebago Medicine Dance," *Journal of American Folk-Lore*, vol. xxiv (1911), pp. 149-208.

<sup>2</sup> "The Assiniboine," *Anthropological Papers of the American Museum of Natural History*, vol. iv, Pt. I, pp. 77 et seq.

relationship of the identical features has never been challenged, while apart from these features there is obvious *divergence*. It is Ehrenreich's group of "false analogies" that supplies us with illustrations of the second type of classificatory error, and this has a direct bearing on the principle of convergence.

Comparing the two types of inadequate classification, we may say that the first type involves the assumption that an organic relationship exists where it does not exist, while the second type of error results from the failure to note that the supposedly parallel elements are organically related to two distinct complexes. In this latter case, then, the parallelism is between logical abstractions rather than between psychological and ethnological realities. Some concrete illustrations will make the matter clearer.

Owing to their theoretical interest, the so-called age-societies of the Plains may properly serve to introduce the subject. J. O. Dorsey reports that among the Omaha there were three feasting societies, composed of old men, middle-aged men, and youths respectively. In tribes of the same cultural area (Arapaho, Blackfoot, Mandan, Hidatsa) other writers have found series of dancing societies evincing a more refined classification by age, admission into any one society being contingent on a payment. Schurtz assumes that the existence of age-grades among the Omaha and other Plains tribes is due to an innate tendency of human society towards an age-grouping, which leads everywhere to similar results. From Graebner's point of view, the existence of so marked a feature as age-grades in a practically continuous area must be explained as due to historical connection. If, on the other hand, we here applied the principle of convergence in the sense defined by Ehrenreich, we should say that the resemblance between the Omaha age-classes and the age-societies of the other Plains tribes is due to the union of general psychic and specific cultural similarities of all the tribes concerned.

As a matter of fact, each of these three interpretations is erroneous. The Omaha feasting organizations are age-classes properly so-called; that is to say, a man belongs to one of the three classes by virtue of his age. But the fact that, say, the Hidatsa societies present the appearance of age-classes, is due to the mode of purchase obtaining in this tribe. The age factor is indeed active, inasmuch as it is customary for age-mates to purchase a society in a body; but there is no established division of Hidatsa society into age-grades, no correlation between age and membership in a certain definite organization. The correlation is, instead, between membership and *purchase*: an Hidatsa belongs to every society of the series which he has purchased, but which has never been purchased of him. A man of ninety may thus hold membership in a young men's society, and under abnormal circumstances a

group of men may acquire a membership which ranks superior to that of an older age-group. To call both the Omaha and the Hidatsa organizations "age-societies" is therefore admissible only if we regard this term as a convenient catch-word which may denote neither psychologically nor genetically related phenomena. The age-factor that we isolate in studying the Hidatsa system is, of course, as a logical abstraction comparable to corresponding abstractions, whether derived from the Omaha system or that of the Masai. In reality, however, it forms part of a context which determines it, and from which it cannot be wrested without completely altering its character. What we find in comparing the Omaha and the Hidatsa systems is therefore a convergence of a type different from that defined by Ehrenreich, but coinciding absolutely with that of his "false analogies," which result from our relative ignorance of the phenomena compared. So long as we knew only that the Hidatsa had societies composed of men of different ages, it was possible to classify them as age-grades proper. With the additional knowledge of the subjective attitude of the natives towards these societies, the justification for such a classification disappears.

What has just been shown for age-grades may be similarly shown for the much-discussed phenomenon labelled "exogamy." It has commonly been assumed that the regulation against marriage within a certain group, no matter in what part of the globe such a regulation may be found, is uniformly the same in principle. Dr. Goldenweiser has recently shown that this is by no means the case. Clan exogamy may indeed be the expression of the feeling that marriage within the clan as such is incestuous; but it may also, as among the Toda and Blackfoot, be a secondary development, the fundamental fact being an objection to marriages between blood relatives. From Dr. Graebner's standpoint there is no reason to differentiate between the primary and the secondary type of clan exogamy. The form-criterion merely tells us that two groups are both exogamous; that in point of exogamy they are identical, and in so far may reasonably be supposed to be genetically related. So far as the criterion of quantity is concerned, nothing would be easier than to bolster up the parallel exogamy by other resemblances. Thus, the Crow social units, which exemplify the clan of "classical" ethnological literature in being exogamous in their own right, bear nicknames of similar type to that of the Blackfoot. Here again the identity of the facts compared is logical, while the facts we are really interested in studying are psychological. The exogamous conduct of the Blackfoot is inseparably linked with his feeling towards blood relatives; the exogamous conduct of the Crow is part of a quite distinct psychological complex. Only by disregarding the characteristic features of exogamy in these two instances do we get an identical *Gedankendin g.*

In this connection it is interesting to discuss the two-phratry system (*Zweiklassensystem*), as Graebner himself makes an extensive use of this concept, suggesting, for instance, an historical connection between the two-phratry organization of Oceania and that of the Northwest Coast Indians and the Iroquois.<sup>1</sup> Before considering such a suggestion, we should have to be convinced that the term "two-phratry system" invariably labels the same phenomenon. Serious doubt is thrown on such a supposition by a consideration of the data collected by Rivers among the Toda. In this tribe the numerical preponderance of one clan is such, that its members can follow the exogamous rule only by marrying most of the members of the other clans, "leaving very few to intermarry with one another." Out of 177 marriages, only 16 were between members of the other clans. As Rivers recognizes, there has thus developed the closest conceivable approximation to a two-phratry system.<sup>2</sup> Yet this result has been achieved by unique historical causes quite distinct from those which brought about such a system where there are merely two intermarrying phratries without any lesser exogamous units.

An instance of similar suggestiveness is furnished by the recent history of the Crow. A visitor to this tribe some forty years ago would have found the male members of the tribe grouped in two social units,—the Foxes and the Lumpwoods. Without any real feeling of mutual hostility, these two units were constantly pitted against each other; for example, taking opposite sides at games, and constantly attempting to outdo each other in warlike deeds. To a superficial observer this division would have appeared similar to that of the Iroquois phratries, though, as a matter of fact, the Lumpwoods and Foxes were not social units with inheritable membership, but military societies. At all events, even a more careful investigator might have been struck by the phenomenon as one comparable with the tendency to the formation of dual divisions, as evidenced in civilized life by the frequency of two dominant political parties. Nevertheless, forty years prior to the hypothetical investigator's advent, he would have found no less than eight societies of the same type.<sup>3</sup> A detailed study of the development of military societies among the Crow shows beyond a doubt that the presence of but two military organizations forty years ago was not due to a primary dual organization, but came about solely through the elimination of the other organizations. A comparison of the Crow conditions with those still more recently found among the Gros Ventre is of the utmost interest. In this tribe the old ceremonial grouping of the men in a

<sup>1</sup> *Anthropos*, iv, p. 1021.

<sup>2</sup> "Totemism, an Analytical Study," *Journal of American Folk-Lore*, vol. xxiii, p. 246.

<sup>3</sup> Maximilian Prinz zu Wied, *Reise in das innere Nord-America in den Jahren 1832 bis 1834* (Coblenz, 1839), v. i, p. 401.

rather large number of small companies representing probably six age-grades has been completely superseded by a division into two organizations,—the War Dancers and the Star Dancers. The tribal and social functions of these societies bear close resemblance to those exercised by the Lumpwoods and Foxes of the Crow, and the spirit of rivalry is equally prominent in the Gros Ventre organizations. But while the dual grouping of the Crow men resulted from a process of elimination, precisely the reverse process took place among the Gros Ventre. The War Dance "is universally stated to be a recent importation from the Sioux, apparently within the present generation;" while the Star Dance is probably an old ceremony independent of the age-series.<sup>1</sup> In the two cases under discussion, then, a dual grouping is beyond a doubt the result of convergent development.

To revert to Graebner's own concepts, we may next consider his category of drums with skin drum-heads.<sup>2</sup> He is careful to enumerate the several Oceanian forms; but as soon as his extra-Oceanian speculations begin, differences of form seem to become negligible. The skin drum of the West African culture-area is described as one of the elements connecting it with Melanesian culture. It is said to appear with all the characteristic modes of securing the drum-head,—viz., by thongs, pegs, and wedges,—though the hourglass shape of the instrument is less frequent.<sup>3</sup>

Probably it would be difficult to find a more offensive example of the misapplication of the form-criterion. The very reference to the hourglass-shaped forms of Africa involves an error of the worst kind. Graebner's authority defines the hourglass drum of Africa as composed of two skin-covered bowls connected by a cylindrical tube. Three sub-types are distinguished, of which two recall the shape of a dumb-bell, while the third differs radically from the two others by the presence of four lugs and profuse decoration, and by the width of the connecting cylinder, which approximates that of the bowls.<sup>4</sup> For convenience of description, Ankermann is certainly justified in creating an hourglass type. But it would be unjustifiable to draw any inference as to genetic relations between the third and the two other sub-types; for quite apart from the elaborate decoration and the four lugs, the third sub-type is not at all similar to the dumb-bell form. It is a psychological commonplace that even congruous geo-

<sup>1</sup> Kroeber, "Ethnology of the Gros Ventre," *Anthropological Papers of the American Museum of Natural History*, v. i, pp. 234-239.

<sup>2</sup> "Ein Element von sehr typischer Verbreitung bieten zum Schlusse noch die *Musikinstrumente* in der einseitig bespannten, meist sanduhrförmigen, bisweilen zylindrischen Felltrommel" (*Anthropos*, iv, p. 770).

<sup>3</sup> *Ibid.*, pp. 1011 et seq.

<sup>4</sup> Ankermann, "Die afrikanischen Musikinstrumente," *Ethnologisches Notizblatt*, 1901, vol. iii, pp. 98 et seq., 53-55.

metrical forms may produce very different psychological effects. It is a fact known to field-workers in America that identical patterns are sometimes not recognized by the natives as identical if executed in different colors. *A fortiori*, we cannot assume without proof, that, where the divergence of form is very great, the native still assembles the varying forms under the same concept. Artifacts differ from organic forms in lacking an innate tendency to variability. If, therefore, we suppose that the lugged (Barotse-Amboella) sub-type developed out of the dumb-bell form, or *vice versa*, we introduce either the hypothesis that some external condition determined the change, or the psychological hypothesis that both forms were originally conceived as of one type. For neither of these suppositions is there the slightest foundation.

If the foregoing argument applies within even a relatively continuous area, its force surely does not diminish when "hourglass drums" of different continents are compared. Indeed, the hourglass drum of New Guinea, as described and pictured by Finsch, Biró, Schlaginhaufen, and others, bears no resemblance to the African sub-types. We must regard the term "hourglass drum" as merely a convenient classificatory device by which may be described objects of diverse origin. The geometrical abstraction defined by the term corresponds to no cultural reality; it develops in different areas by convergent evolution.

As a matter of fact, the hourglass type which at least presents a semblance of morphological classification plays a very subordinate part in Graebner's treatment of the skin drum; for under the category of skin drums—and accordingly as evidence of a cultural connection between Oceania and North America—are cited the ordinary dancing-drum and the Midewiwin drum of the Ojibwa.<sup>1</sup> Thus the form-criterion is completely abandoned by its champion.

It is true that Dr. Graebner, in his treatment of this subject, attaches considerable weight to the method of securing the drum-head,—whether by thongs, pegs, or wedges (*Schnur-, Pflock- und Keilspannung*). This leads to an important question. How many ways of fastening a skin membrane to a drum are conceivable? Very little reflection is required to show that the number is exceedingly limited. Indeed, the wedge system, being only a sub-type of the *Schnurspannung*, is not entitled to a special position on logical grounds, though from a comparative point of view it is incomparably the safest criterion of relationship. We must here apply what Dr. Goldenweiser has called, in conversation with the author, "the principle of limited possibilities," which has recently been thus defined: "The theory of convergence claims that similar ways *may* (not *must*) be found. This would be a

<sup>1</sup> *Anthropos*, iv, p. 1021.

truism if there existed only one way of solving this problem; and convergence is obviously the more probable, the fewer the possible solutions of the problem."<sup>1</sup> In the case at hand, it cannot be taken as a sign of genetic connection that some African and some Oceanian tribes use pegs for fastening a drum-head, because the number of available ways is very small *if classified in a manner that abstracts from all definite characteristics.*

This point is illustrated most clearly where the logical classification involves a dichotomy of the universe. A well-known writer has discussed the origin myths of primitive folk, and found that some involve a theory of evolution, others one of special creation. No sane ethnologist would infer from this that all the myths of either type were historically connected. To choose a somewhat more drastic illustration. Acquired biological traits must either be inherited or not inherited: consequently an expression of opinion, whether consciously or unconsciously bearing on the subject, must fall into either category. Many primitive tribes have myths recounting how in the remote past a certain animal met with some adventure which caused it to assume some biological peculiarity now noticeable in its descendants; nevertheless it would be absurd to accept this tacit assumption of transmission as a parallel of anti-Weismannism. Countless examples of a mode of classification rivaling in absurdity the hypothetical instance last cited are furnished by histories of philosophy. Too frequently the historian utterly neglects the processes by which conclusions are reached, and groups thinkers exclusively by the nature of their conclusions, which are labelled by descriptive catch-words. The identification of a philosopher as a monist or dualist, idealist or realist, is undoubtedly a labor-saving mode of characterization; but unfortunately it precludes a deeper comprehension of the thinker's philosophic individuality. A differentiation of social systems on the basis of maternal and paternal descent, such as Graebner has undertaken, is justifiable within a limited area, where historical connections can be definitely demonstrated. Outside such an area it can have no comparative significance, because descent cannot be reckoned otherwise than in either the maternal or the paternal line, or in both.

#### THE POSSIBILITY OF GENUINE CONVERGENCE

The foregoing discussion has indicated the nature of the errors due to premature classification. The frequency of such errors, and the readiness with which they are committed, surely justify the greatest caution in identifying apparent homologies in the cultures of tribes not known to be historically related. The first question we must ask is, not how the trait could have travelled from one region to another,

<sup>1</sup> Boas, in *Science*, 1911, p. 807.

nor even whether it could have originated independently through the psychic unity of mankind. Our first duty is rather to ascertain whether the resemblances are superficial or fundamental. For example, if we discover that the *manang bali* of the Sea Dyaks corresponds in the most striking manner to the *berdache* of the Plains Indians,<sup>1</sup> we should not straightway identify the two institutions and invoke the principle of psychic unity or that of historical connection. Psychic unity would only explain the fact of a pathological variation, which seems to occur everywhere with a certain frequency. It does not explain why in but two particular areas this variation should lead to a marked social institution. Neither can historical connection be postulated in the absence of a tittle of evidence for either genetic relationship or transmission. The advocate of convergence in the sense here proposed will simply await a fuller determination of the facts. If closer investigation should establish an absolute identity, the fact of identity would stand, but would stand unexplained.

But in many instances the identity of the cultural elements compared seems to be far more than an abstract possibility. The eye-ornament of the northwest coast of America is identical with that of Melanesia. For all practical purposes the star-shaped stone club-heads of New Guinea are identical with those from Peru. To put the case in the most general form, wherever we are dealing with objects which can be fully determined by an enumeration of their visible or sensible traits, there is the possibility of proving objective identity, as indicated by the examples just cited. However, there is an important consideration which cannot be neglected in this connection. The sensible traits of an ethnographic object may completely determine its character from the standpoint of the curiosity-dealer, but never from that of the scientific ethnologist.<sup>2</sup> For the latter a material object has a purely symbolical function: it represents a certain technique, an artistic style, a religious or social usage. In this sense it may be rightly said that "material" culture does not exist for the ethnologist, for the very word "culture" implies a psychological correlate, or rather determinant, of the material object. According to Pechuel-Loesche, the same representation of a human figure that in one West African specimen is nothing but a product of art industry, becomes, when endowed with certain magical powers by virtue of incantations or the application of sacred substances, a fetich. Exactly the same purpose, however, may be served in the same tribes by the most inconspicuous objects of nature. A purely objective comparison would here lead to an utterly erroneous classification. It would wrest the factors

<sup>1</sup> Gomes, *Seventeen Years among the Sea Dyaks of Borneo* (Philadelphia, Lippincott, 1911), pp. 179 et seq.

<sup>2</sup> Cf. Boas, in *Science*, vol. xxv (1907), p. 928.

studied out of their organic context in quite the same way as an identification of the cultural traits discussed in the preceding section; it would neglect the very factors that we are most interested in studying.

As has been pointed out by American archæologists, the application of the form-criterion is insufficient in determining the antiquity of an archæological object; for the latter may not be at all the completed object designed by the worker, but a mere "reject."<sup>1</sup> Yet objectively the rejects coincide absolutely with the finished products of a lower culture. The difference lies in the cultural contexts of which the objects are elements: the resemblance may be perfect from a purely external standpoint; nevertheless it represents, in Ehrenreich's terminology, not a genuine convergence, but a false analogy. A most suggestive fact pointing in the same direction has been ascertained in Central Australia. The natives of this area use implements, some of which fall morphologically under the category of paleoliths, while others are neoliths. Investigation has shown that this morphological difference is a direct result of the material available for manufacture. Where diorite is available, the natives manufacture "neolithic" ground axes, in other cases they make flaked implements practically as crude as those of the ancient Tasmanians.<sup>2</sup> The manufacture of "neolithic" implements in Central Australia and elsewhere thus forms another instance of convergence, — a classificatory resemblance due to heterogeneous conditions. It is true that Graebner does not ignore the possible influence of material on form,<sup>3</sup> but he fails to show under what circumstances the ethnologist should seek to correlate morphological resemblance with the nature of the material. The form-criterion by itself does not tell us that diorite lends itself to "neolithic" workmanship, that bamboo bows are necessarily flat, that basalt furnishes the only material available for axe-manufacture in certain regions. Under what conditions should we be satisfied with formal coincidence as a proof of genetic relationship, and under what conditions should we inquire as to the possible influence of the available material?

The case of the eye-ornament adds force to the general argument. As Graebner might have learned from Ehrenreich's article (*l. c.*, p. 179), Boas has shown that the eye-ornament of Northwestern America results from a peculiar style of art, which, so far as we know, does not occur in Oceania; that is to say, the objective identity is again deceptive, because it is an identity established by wresting a part of the phenomenon studied (the visible pattern) from the midst of its cultural context. Here it must again be stated that Graebner does not unqualifiedly uphold the omnipotence of the morphological principle.

<sup>1</sup> Mason, *The Origins of Invention*, p. 124.

<sup>2</sup> Spencer and Gillen, *The Northern Tribes of Central Australia*, 1904, p. 635.

<sup>3</sup> *Methode der Ethnologie*, pp. 145, 117.

He rejects Von Luschan's speculations on the head-rests of New Guinea; he regards Schurtz's theories of the eye-ornament as "weniger phantastisch, aber doch auch übers Ziel geschossen;" he stigmatizes Stucken's attempt to trace all celestial myths to Babylon as an example of the neglect or unmethodical application of the form-criterion (p. 118). Unfortunately, he does not explain what is meant by an unmethodical or fantastic application of the form-criterion. As has been shown, the criterion of quantity is a measure of the historical connection between cultures, but can never decide as to the identity of doubtful traits. If all the other elements of Oceanian and northwest American culture were identical, the fact would prove nothing as to the identity of the eye-ornament in the two areas.

We are not always, indeed we are very rarely, in the fortunate position of knowing most of the determining conditions of an ethnological phenomenon. In the case of the rejects, of the central Australian "neoliths," and of the eye-ornament, we happen to be in possession of the facts; and from these instances we learn that morphological identity may give presumptive, but does not give conclusive, evidence of genetic relationship. It is conceivable that if we could determine the history of the South American paddles, which Graebner connects with Indonesian and Melanesian patterns,<sup>1</sup> we should find them to be genetically related; but we cannot bar the other logical possibility of independent origin, for it is likewise conceivable that each of the homologous features of the paddles originated from distinct motives and distinct conditions.

#### CONCLUSION

The doctrine of convergence, as here advocated, is not dogmatic, but methodological and critical. It does not deny that simple ethnological phenomena may arise independently in different regions of the globe, nor does it deny that diffusion of cultural elements has played an important part. It does not even repudiate the abstract possibility of the independent origin of complex phenomena (genuine convergence of Ehrenreich), though so far the demonstration of identities of such a character seems insufficient, and their existence would be unintelligible. The view here propounded demands simply that where the principle of psychic unity cannot be applied, and where paths of diffusion cannot be definitely indicated, we must first inquire whether the supposed identities are really such, or become such only by abstracting from the psychological context in which they occur, and which determines them, — whether, that is to say, we are comparing cultural realities, or merely figments of our logical modes of classification. A rapid survey of the field has sufficed to show that in many cases where some would invoke the principle of psychic unity, and others that of

<sup>1</sup> *Methode der Ethnologie*, p. 145; *Anthropos*, iv, pp. 763, 1016, 1021.

historic connection, the problem is an apparent one, which vanishes with a better knowledge and classification of the facts.

Dr. Graebner's ambitious attempt to trace historical connections between remote areas cannot be dismissed wholesale, on the basis of the foregoing criticisms. What has been shown is simply the necessity for a critical use of ethnological concepts, and their occasionally quite uncritical use of Graebner. Even tangible specimens, it appears, cannot be studied apart from the culture of which they are a product. In the investigation of social and religious usages, where the subject-matter is itself psychological, the exclusive consideration of the form-criterion, to the detriment of the subjective factors involved, can lead only to disastrous results. Ethnology is a relatively young science, and it is natural that the mode of classification in vogue among ethnologists should have a pre-scientific tang. But the time has come to recognize that an ethnologist who identifies a two-class system in Australia with a two-class system in America, or totemism among the Northwestern Indians with totemism in Melanesia, sinks to the level of a zoölogist who should class whales with fishes, and bats with birds.

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